

magnitude of a star, the light of which, if diffused over a circle one minute of arc in diameter, would be equal in brightness to that of the nebula.

The star employed as a standard in the present instance was *a Cygni*. The comparisons were made upon three evenings, and three observers took part in the measurements. The number of determinations is six. If we assume the magnitude of *a Cygni* to be 1.7, as in the *Durchmusterung*, that of the nebula is 4.6 on the system just explained. The average deviation of the separate results is 0.4, and the probable error of the mean 0.2. The scale of stellar magnitudes here adopted is that of Pogson, in which the ratio of light corresponding to one magnitude is that the logarithm of which is 0.4. Accordingly, it appears that the brightness of *a Cygni* would be equal to that of the nebula, if the light of the star were diffused over a circle 3.8 in diameter.

In the position angle 140° , the diameter of the nebula is about $11''$, and the diameter perpendicular to this is about $8''$. The border of the nebula is not sharply defined, and the fainter light around it is not very regularly distributed about its central portions. In a smaller telescope it would probably look smaller and more nearly circular.

From the measured dimensions and brightness of this nebula, its total light may be computed. The result is that, according to these observations, we receive 590 times as much light from *a Cygni* as from the nebula. Hence, regarding the nebula as a star, its magnitude may be expressed by $1.7 + \frac{2.77}{0.4}$, or 8.6.

The magnitude assigned to it in the *Durchmusterung* is 8.5. This close agreement must of course be regarded as accidental.

Like most of the planetary nebulae observed here, this nebula shows a faint continuous spectrum, not due to the light of the sky, in addition to the lines denoting its gaseous character. This continuous spectrum is largely due to the nucleus.

EDWARD C. PICKERING

Cambridge, U.S., January 24

Electricity of the Blowpipe Flame

COL. ROSS'S experiment on the above subject seemed of such importance that I thought it advisable to repeat it, and it may be of interest to some of your readers to hear of the result and of the way in which my experiment was conducted.

A compass in a closed box, to prevent the influence of air currents, was placed close to a brass Herapath blowpipe, and after the position of the needle was noted the gas was lighted and air was blown through the flame; no deflection of the needle was observed. As the compass is an old one and there was probably some friction on the pivot, it was replaced by a piece of magnetised watch-spring attached to a mirror, and suspended in a glass case by a single silk fibre; this apparatus being placed on a stone slab, light from a lamp was reflected from the mirror on to a screen. The arrangement was so delicate that the needle was set in oscillation by the movement of the iron rod connecting the blowpipe with the treadle; so, to avoid any possible disturbing cause, the air was supplied by water pressure from a copper gas holder. When the jet was brought near the needle, the flame being either in the magnetic meridian or at right angles to it, not the least movement of the spot of light was perceived, although the screen was at a distance of about eight feet from the mirror.

As this result is so much at variance with that of Col. Ross, it would be interesting to know exactly how his experiment was performed.

HERBERT M'LEOD

Cooper's Hill, February 4

Triassic Footprints

In the *Quarterly Journal* of the Geological Society for August last there is an interesting notice by Mr. Sollas, accompanied by a figure, of a set of footprints from the Triassic beds of South Wales. These footprints Mr. Sollas says he has compared with those of the emu taken in modelling-clay; and so complete was the agreement that, other considerations out of the question, he would not have felt much hesitation in declaring for the avian, and indeed ratitous, character of the animal that produced them; but that because no remains of birds have occurred in the trias of the south-west of England, while those of reptiles have, he refers them to either *Thecodontosaurus* or *Palaeosaurus*.

I wish, therefore, to call attention to the fact that in these

footprints there is shown that character of the crossing of one leg over the other, and of turning out the toe, which persons who have kept poultry may have noticed as conspicuous in the walk of the domestic fowl; that is to say, it places the foot, not directly forward, but across the opposite leg, turning the toe well out. Now this is distinctly shown in the relative positions of these Triassic footprints. The first, or lowermost in the figure, is that of the right foot, and the toes point to the right; the next (2) is that of the left foot, and crosses the median line of the animal's path, and the toe of this (for only the middle one remains unobliterated), points well to the left; the third, being that of the right foot, crosses the median line in the same way, its toes pointing well to the right; but the fourth (left), though it thus crosses, has not the toe turned out, because the animal at that point began to bend its course to the right hand.

This track is thus, I venture to say, one made by the jaunty step of the light-limbed bird, and not by the slouching stride of the heavy-limbed dinosaur, even if this kind of reptile did (as has not yet, notwithstanding its ornithic affinities, been shown) walk erect, and exclusively on two legs; and I am induced to trouble you with these remarks, because just twenty years ago (*Quart. Jour. Geol. Soc.*, vol. xvi. p. 328) I contended that the existing *Ratite* and other wingless (or, more accurately, flightless) birds are the direct, and but little altered, descendants of those which inhabited Triassic continents in the southern hemisphere, of which one portion, that formed by Australia and New Zealand, has been preserved in complete, and other portions, such as South Africa and South America, in less complete isolation since that remote period; and it seems to me that the footprints figured by Mr. Sollas furnish very satisfactory evidence of the case.

CHARLES V. WOOD, Jun.

Martlesham, near Woodbridge, January 30

Rainfall in the Tropics

MY studies on the distribution of rain on the earth have often caused me to regret our want of knowledge about the quantity of water falling on the oceans, especially in the tropics. The observations on the continents and large islands are very apt to mislead us as to what takes place on the open sea. As there seem to be very great difficulties about observing rain-gauges at sea, I have thought it would be possible to gain some insight into the matter by placing rain-gauges on the smallest and lowest islands to be found on the ocean, the meteorological conditions of which differ but very little, if at all, from those of the ocean. In the Pacific such islands are to be found in plenty; in the Atlantic I would especially recommend the island of St. Paul 4° N. and $29\frac{1}{2}^\circ$ W.; in the Indian Ocean, the Southern Maledives, the Chagos, and Keeling Islands, &c.

The rain-gauges for this purpose should be made of strong metal, the lower part, instead of the ordinary glass measuring-vessel, being also of metal. Such rain-gauges could be put on islands, especially uninhabited, and taken up and the amount of water fallen measured after some months, or even a year or more. The measurement would be but a rough one, as the evaporation could not be strictly accounted for, and we would certainly know very little as to the distribution of rain during the year; but with all these drawbacks, even an approximate knowledge of the quantity of water falling in strictly oceanic climates, far from the disturbing influence of land, would be very important for meteorology. Even a few figures as to the total annual rainfall in parts of the ocean, which are for some months included in the "doldrums," and those where the trade-winds blow steadily the whole year, would very much increase our knowledge, more than a great number of observations taken on mountainous islands, where local conditions modify the quantity in the extreme.

I refrain from further practical details, as these will be better provided for by British meteorologists and seamen, in case they should accept my suggestion.

A. WOEIKOF

St. Petersburg, January 21

Mountain Ranges

THE reply which Mr. H. B. Medlicott has made to me in *NATURE*, vol. xxi. p. 301, seems only to obscure rather than set aside or remove my objections. In the second sentence it is said that I "take geologists to task for not making their descriptions to fit in with my delineation of purely superficial features." But my complaint was based, not on my delineation, but on a trigonometrical survey; and it was caused by a description—not of

the geology, but of the physical geography of India, in connection with a map of its hill ranges, that has nothing geological about it. It is in this expressly geographical part of the manual that I find the greatest range of snowy peaks in the world omitted from a geographical notice and delineation of the Himalaya. I did not allude at all to geology.

Mr. Medlicott contends that the omission was due to the irrelevancy of the great range to the matter in hand. But how can a great range of the Himalaya be irrelevant to a geographical description of that mass, or to a special map of the hill ranges of India? And why should a prominent and leading feature be treated as a mere incident? In fact the omission was plainly due to the survival of an old error or "antiquated theory," which confused the snowy peaks seen from the Indian plains for the most part with the water-parting of the Sanpu and Ganges basins, although the latter really forms a distinct but parallel range further to the north. In these days a clear understanding of the superficial or geographical aspects of the mountains on the frontier of India cannot be overrated. The statesman, the warrior, and the trader alike stand in need of it; and misleading or confused representations of the subject may become of serious moment. The ignored range is indeed to a great extent the limit of the Tibetan Plateau and of the Chinese Empire, the relations of which with India are rapidly rising into importance.

Mr. Medlicott's appeal to "the great gneissic axis" is not less unfortunate than the argument which he derives from "irrelevancy." If "the great gneissic axis" divides on the west of the Sutlej, it may be presumed to be intact on the east of that river, where in consequence it would be the more entitled to delineation and notice. But the only parts of the Southern Himalaya inserted in Mr. Medlicott's map of the Hill Ranges, are the Pir Panjal and Dhauladhar, on the west of the Sutlej. Is there any ground for identifying "the great gneissic axis" with the Northern Himalaya, which alone is delineated east of the Sutlej, in preference to the Southern Himalaya which is omitted? It is enough to say that neither of those ranges has been sufficiently explored, to admit of a general conclusion on the subject. Therefore it is fair to add that even geologists must refrain for the present from accepting Mr. Medlicott's dictum in that respect.

Mr. Medlicott's penultimate sentence baffles my best efforts to understand it. It seems to be meant to be applicable somehow to the region between the Indus and Sutlej.

In conclusion I can find no good ground for treating the views of geographers and geologists as wide apart, merely because a great geographical fact has been neglected in an important geological work; and I hope that the omission will be rectified in future editions.

TRELAWNY SAUNDERS

On Halley's Mount

PERMIT me to mention two suggestions which have been made with reference to the article "On Halley's Mount" in NATURE, vol. xxi. p. 303, viz.:—

1. That some mention should have been made therein relative to Dr. Halley's official investigations (*vide Phil. Trans.*, vol. xvii. p. 960, 1693).

2. That it was *not* at Dr. Halley's private expense¹ that the "Principia" was published, although it was in consequence of his urgent persuasion that Newton produced his great work (*cf.* Preface to the "Principia").

It may be remarked that there is a biographical sketch of Edmund Halley in Mr. Crookes's *Monthly Journal of Science* for February, and that the Astronomer-Royal has signified his hearty approval of the idea of the proposed monument in St. Helena.

THE WRITER OF THE ARTICLE "ON HALLEY'S MOUNT"
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"A Speculation Regarding the Senses"

IN a letter bearing this title (NATURE, vol. xxi. p. 323) your correspondent, "M.," while indulging in a most extraordinary "speculation," observes that it is "not without some encouragement in actual fact." He then adds: "The ascertained facts of clairvoyance and mesmerism are what I have more especially in view," &c. Now, whatever may be the case with clairvoyants, I think, to quote from "M.," that it must certainly "require some peculiar state of mental calm" to enable a man, when writing in a journal professedly scientific, thus quietly to assume

¹As inferred from Whewell's "History of Inductive Sciences."

the truth of all the astounding class of phenomena to which he alludes as "ascertained facts." Clairvoyants, spiritualists, *et hoc genus omne*, often complain that scientific men are arrogant in their treatment of, or allusions to, the alleged marvels of the modern *science*; and if we have regard to the jaunty manner in which Dr. Carpenter rides his favourite hobby along "the high priori road," I do not deny that the spiritualists have sufficiently good ground for complaint. But let them not meet arrogance with arrogance, or speak about facts which, at the best, are highly doubtful as facts which have been "ascertained."

My object, however, in writing this letter is not controversial. I desire merely to represent to "M.," and any other of your readers who may believe in the alleged phenomena of clairvoyance, that it is their duty to have these "facts" properly sifted, examined, and published. I have myself taken a good deal of trouble to investigate the subject, and, while meeting with a vast amount of humbug, have also met with one or two things that I am unable satisfactorily to explain. I therefore desire to prosecute my researches in this direction, without either bias or prejudice, should I be able to meet with suitable material. If "M." and his friends are right, and if I should satisfy myself that they are so, I should give a wide publicity to my methods and my results. If the phenomena should admit of repetition, I should have them witnessed and attested to by a selected number of the leading scientific men of the day. It would then be time for "M." to speak about such "facts" as "ascertained."

Here, then, is a fair offer by "a man of science" to investigate any or all of "the powers of darkness" without any feelings of animosity against them. Will any clairvoyant or spiritualist who really believes in his own belief supply me with an opportunity of so doing? Any letters addressed to the care of the Editor of NATURE will be forwarded to me.

F.R.S.

Perforated Stones in River Beds

TRAVELLING some months ago among the Cumberland lakes, I was walking with a friend in advance of our conveyance through a narrow road, when my attention was suddenly arrested by the presence of some interesting shells and stones on the window-sill of a peasant's cottage. Stopping to admire them, or rather having taken some of them up in my hand, the woman of the house—an intelligent person—came out, whereupon I apologised for my seeming rudeness, and asked where she got them. She at once accepted my apology, and added that they, pointing to the shells and stones, were often looked at by other travellers. She further added that they were common enough in the Derwent River hard by, and she made no difficulty at all about accepting sixpence for the two of them I selected.

Now as I have travelled a good deal in the public service and otherwise, and seen many mountain and other streams in my day, without ever meeting any of these perforated stones, I would like to know if they occur elsewhere, and if so under what circumstances. The Derwent, a comparatively small and gentle stream, flows, as we all know, through the beautiful valley of Borradale into the pretty lake of the same name, near Keswick. I do not know anything of the geology of the district, but there are slate quarries and lead mines in the vicinity, and one of my stones partakes indubitably of the former quality. The other is as clearly a piece of granite, and if water be the sole tunnelling agent in these substances, both well illustrate the truth of the old Latin phrase, "Gutta cavat lapidem, non vi, sed sæpe cadendo."

Another thing that struck me in connection with them was the extraordinary likeness of one of them, at least, to the stone axes or hatchets (I forget just now the technical name) figured by Sir John Lubbock in his "Prehistoric Times." This was so striking and obvious that, holding up the specimen, I said to my friend—a gentleman connected with the Press—"Surely Lubbock must have made a mistake, and taken one of these for a prehistoric implement." Further observation only tends to confirm this first impression, and I shall be glad to hear if any similar doubt has occurred to others on sight of these objects. I will also be anxious to hear if they are as common in the Derwent or other rivers as this woman's language would imply, and I will otherwise feel obliged for such information respecting them as the courtesy or curiosity of your readers may enable them to supply.

Warrington

WM. CURRAN

Politics and Science

THE Duke of Somerset, after "considering all the oppressions that are done under the sun," writes about them all,